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AND

MAGAZINE OF NATURAL HISTORY.

"……………… per litora spargite muscum,
Naiades, et circùm vitreos considite fontes :
Pollice virgineo teneros hìc carpite flores :
Floribus et pictum, divæ, replete canistrum.
At vos, o Nymphæ Craterides, ite sub undas ;
Ite, recurvato variata corallia trunco
Vellite muscosis e rupibus, et mihi conchas
Ferte, Deæ pelagi, et pingui conchylia succo."
N. Parthenii Giannettasii Ecl. 1.

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I.—*On the Formation of the Flints of the Upper Chalk.*

By J. Toulmin Smith, Esq.

[With a Plate.]

VARIOUS theories have, at various times, been proposed to explain the very curious and interesting phænomena connected with the presence of flints in the upper chalk. Having for some time paid close attention to the facts, from which alone any true theory can be framed, I would now offer a few observations on some of the more prominent of those theories.

The theories which have been proposed have had very different objects, which it is desirable to distinguish clearly at the outset. Thus, while Ehrenberg and Dr. Turner have suggested explanations of the *origin* of flint itself, without entering into detailed observations on the particular modes and forms in which we now find it in the chalk, Dr. Buckland and Mr. Bowerbank have endeavoured to explain, not the *origin* of flint, but the forms and modes in which it is now found.

Disposed as I am to regard the explanation offered by Dr. Turner*, combined with other sources known to exist, of the *origin* of the siliceous fluid as sufficient and satisfactory, the suggestions of Ehrenberg being, at any rate, applicable in particular cases and to a very limited extent only, our attention will at present be principally confined to the theories which attempt to explain the forms and modes in which the flint is now found.

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* See "Lecture on the Chemistry of Geology," Philosophical Magazine, vol. iii. (1833).

Of these, the theory of Dr. Buckland, that flint and chalk being deposited together in the form of viscid fluids the former separated itself from the latter according to the well-known laws of chemical affinity, is of too indefinite and general a character to be readily applied and tested in individual cases, though, as we shall find, such opportunity does sometimes occur. The theory of Mr. Bowerbank alone proposes a distinct and definite explanation of all the forms and modes in which flint, either in nodules or tabular masses, is actually found. To that theory therefore I will direct chief attention, the more so as it has very recently been advocated in this Journal*, and as I believe the views expressed in Mr. Bowerbank's published paper on the subject† are still held by their author, and they come from him with an authority so great as to claim the most careful and candid examination.

Mr. Bowerbank's views may be shortly stated to be, in his own words, "that the common tuberous flints, the horizontal tabular flints, and those forming perpendicular or oblique veins, were ALL produced by the same agency," namely, IN ALL CASES from sponges, of which those flints occupy the exact places, though of the sponges themselves but small remains generally exist (see pp. 183 and 186 of the paper referred to).

It is not my intention to dispute the particular facts stated by Mr. Bowerbank as applying to the cases observed by him. What I undertake to show is that those, admitting their correctness, are not *all* the facts, and that many others exist wholly inconsistent with the conclusions which Mr. Bowerbank would draw. And it is here well deserving of remark, that, while Mr. Bowerbank finds in such abundance on the exterior of all flints sponge spiculæ and evidence of the mouths of the excurrent canals of sponges, Ehrenberg finds the same exteriors to consist almost entirely of the skeletons of Infusoria! No one accustomed to the use of the microscope can be otherwise than aware how much very minute objects seen under a high power are apt to assume a character in accordance with preconceived notions; and when we find such careful observers as Mr. Bowerbank and Ehrenberg thus differing altogether and in so very marked a manner in the results of their examinations, it may be allowed us, in all humility, to call in the aid of other classes of facts to clear up the mystery, and this I now proceed to do.

I fully admit that spiculæ are not uncommonly found in some flints, but they are most assuredly not always found; in some flints they are very numerous, while in others from the same spot they are exceedingly rare, and in very many wholly wanting. Now these sponge spiculæ are indestructible. The destruction of the structure of the sponge, which this theory requires as a necessary postulate, would not destroy them. How then is it that they are thus variably present? And it is important to remember that similar spiculæ are also found in the chalk itself.

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* By Prof. Ansted, 1844.

† Geol. Trans. vol. vi. 181.

It may be remarked, in passing, that this theory, if true, presents us with a phænomenon still more extraordinary and unaccountable than that which it is called in to explain. It is taken for granted that the flint itself is a foreign substance which has filled the places formerly occupied by sponges. Now how happens it that flint should be found in such places only? and this restriction, be it observed, besides being expressly made by Mr. Bowerbank, is necessary to the existence, in any shape, of the theory. If it is once admitted that flint is ever, or may be even in a single instance, found elsewhere, the theory ceases to be an explanation of the phænomena, and becomes of no value to the philosophical inquirer. Now, can it be shown that silex has any peculiar affinity for either the animal substance or the horny skeleton of sponges? The contrary is known, as matter of fact, to be the case. Facts palæontological as well as recent might be cited in abundance in disproof of this necessary postulate of the sponge theory. I have undoubted sponges in my possession from the chalk, which, instead of being wholly silicified, are in *part* so, and in *part* still in the chalk, while the flint is otherwise extended beyond the boundary of the sponge. Such facts disprove the alleged special affinity. But if we turn to events going on before our eyes, we find that not only is silex abundantly secreted on the leaves and stalks of the *Plumbago*, and at the joints of the bamboo, and in many other instances, but the plants growing near the Geysers in Iceland, as well as near the hot springs in some of the Azores, are incrusted with silex, in which many are completely imbedded, and that without the presence or neighbourhood of any sponge whatever. These facts alone, apart from those of a like nature which will presently be named, and which every one must feel to be clearly analogous, are fatal to the theory. They show analogous results produced at the present day by a wholly different set of causes from those suggested by the sponge theory and necessary to its maintenance.

It is admitted that it is very rarely, if ever, that the reticulated tissue which is conceived to have belonged to sponges is found throughout any considerable portion of the flint; that often but little of such tissue is found; often no trace of it at all. Now these facts want, but have not had, explanation. How is it that the bodies themselves whence the flints are said to derive their forms are almost wholly, often wholly, gone, and yet in those very flints are preserved structures, as we shall see, in endless variety, every whit as delicate and beautiful as the sponges themselves, and that too of bodies which, according to the very assumption of the theory, were themselves dead before the sponge was formed over them?

But it may further be asked, how happens it that there are found in flints *fragments* perfect and in beautiful preservation of the reticulated tissue, while all the rest is wanting? This is inconsistently enough an admitted fact,—a fact itself proving that the destruction of this tissue, assumed to have taken place in all but these fragmentary places, is not a necessary or easily explainable fact. These fragments generally show under the microscope a clear and distinct *torn edge*, not a going-off into gradual indistinctness as would be the case if they were the remnants of a large body formerly filling the place of the flint. I can readily understand that fragments of sponges might float about in the ocean mud and become, with other organic remains, imbedded and preserved in flint. We find precisely such fragments in the chalk also. Such a view fully accounts for the presence of these fragments in the flints, while their presence in that fragmentary state is totally inconsistent with the idea of the whole mass of flint having ever been made up of such tissue.

Moreover, perfect sponges do exist among the flints. These however assume a totally different aspect and form to ordinary flints. I have several specimens, in which the general form of the sponge, with its roots, is perfectly preserved, and the structure very beautifully displayed on fracture; and, placed side by side with a fine specimen of recent sponge, the one seems but the solidified representation of the other. Not so the flint nodules or tabular masses. They assume every variety of fantastic form, while it is admitted that roots, or traces of them, are not to be found.

We advance now to another and distinct branch of the inquiry; namely, as to the evidence afforded by the organic remains, undoubtedly other than sponges, which are found in flints: and I think we may derive equally conclusive evidence from this class of facts.

Where, in recent sponges, do we find the innumerable quantities of shells and other large objects that we find in the chalk flints? In a specimen now before me, by no means a picked one, I find projecting from the exterior two Plagiostomas, a Terebratula, a Pecten, twelve Ostreas and several Serpulas; all these obvious; besides innumerable smaller shells and several portions of a quadrangular Ventriculite. This specimen is about 8 inches by 4, unbroken, and of most fantastic form. Another, about 4 inches by 3, and containing one Ventriculite which it just encases, presents two beautiful Dianchoras and at least eight Ostreas, besides many small shells. These are lying on the external surface, just sunk, as it were, in the flint, as they would sink in water, but not at all covered*. No hollow of a sponge could have retained them thus.

I have seen, in Mr. Bowerbank's valuable collection of sponges, a specimen in which one small shell is imbedded: this may have happened in casual instances with small dead shells, but where can it be found, in recent sponges, from the most favourable spots, that they are full, as we find the flints full, of bivalves large, numerous and perfect, and apparently living when enveloped?

It is assumed by this theory that the sponges grew over the shells and other organic objects which lay on the surface of the mud. But the observed facts are wholly at variance with this assumption. The organic bodies are found, not on the under surface of the flints only, but equally on all parts of the surface and in all parts of the inside. Now how can this be accounted for? Shall we suppose that the shells, &c. sank down from above and rested on the sponges till grown over, unaffected by their natural tendency to fall to the actual bottom on the slightest motion in the water? What follows then? necessarily this: that the upper surfaces of the *tabular* masses would, far more than any other flints, display these remains, as such shells, &c. would be far less liable to roll off the assumed sponges of this form. But do the facts support this necessary conclusion from the sponge theory? Assuredly not. It is the nodular flints in which are found most of the organic remains of the class alluded to. Thus on the flat surfaces, off which shells, &c. could not fall, they are not found in any abundance, while on the smaller and nodular masses, of all sizes, off which the least motion, if not their natural gravity, would cause them to fall, they are found in great abundance! The same result follows from the observation of the fact, that on the *under* side of the upper part of the goblet-shaped flints which inclose Ventriculites (and the upper and under surfaces of which can therefore be positively known), shells and other remains, not originally affixed to the surface of the animal, are found, which therefore could never have been either lying on the ground or have fallen from above. These two sets of facts are equally conclusive against the sponge enveloping theory.

The Echinites alone, extensively examined, afford conclusive evidence against the sponge theory. These are very frequently indeed found in the very centre of flints. They are sometimes found with spines affixed, and therefore alive or with undecomposed soft parts when entombed. The masses of flint to which they are affixed are very frequently not attached to either of the large orifices of the shell, but to some part of the sides, while the shell is entirely filled with flint and both orifices closed. Mr. Bowerbank states that, when the shell is not entirely filled with flint, in "the space thus unoccupied by the flint was *always* included one or both of the large orifices of the shell*." I do not find this fact in any degree borne out by my own observations. I have specimens at this moment before me in which the reverse is the fact,—both orifices being closed though the Echinite has never been filled with flint. Many other illustrations of the clear inapplicability of the sponge theory to the case of Echinites might be given.

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* See note, p. 7, for explanation of this fact.

It is frequently the case that the remains of a zoophyte are found in the exact centre of a flint which is externally round. In some localities these abound, and they occur from the condition of a very friable substance, perfectly preserving the structure however, and merely coated with a layer of flint, to the same body perfectly solid and silicified throughout, though still clearly showing the structure. It is easy to understand that, in particular classes of objects (such *e. g.* as some of the beautiful objects inclosed in the Wiltshire flints) and under particular circumstances, the affinity for the siliceous fluid might not be sufficiently great, or the rapidity of its solidifying too great, to allow of its penetrating the body round which it formed. In such cases we should necessarily find, as we do, that the soft animal substance, not being penetrated, has decayed†, leaving only what we find,—the hard and, so protected, indestructible parts; where it was wholly soft animal matter a mere hollow is found. The extent to which penetration extended will of course affect the facts exhibited. The explanation offered by Mr. Bowerbank of the Wiltshire flints is clearly inadmissible. Had they been dead and sponges grown over them, it is obvious that they would not, as now, have been *loose* within the flints, but solidly encased, as shells and corals are found.

I have an Asterias in the centre of a flint. Teeth, pieces of wood, &c. are often found in flints, which could never have become entangled within the meshes of a sponge. It will be said that the sponge may have been built over them. This, though highly improbable, may be possible; but there are cases to which such an explanation cannot apply, in which flint is found in situations to which it is impossible that, on the sponge theory, it could have gained access.

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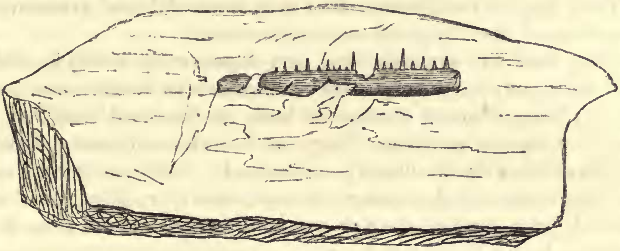
* *Loc. cit.* p. 186.

† I believe it may be stated as an invariable rule, that there is an orifice, large or minute, in all such cases, through which the gases evolved might escape.

The specimen of Mososaurus (*Leiodon* of Owen *), discovered by Mr. Charlesworth, exhibits the pulp cavities of the teeth partially filled with flint. This is sufficiently remarkable; but those teeth being large, and the cavities not being wholly *filled* with the flint, it may not perhaps, alone, be considered as absolutely conclusive. What is wanting in this case is however fully supplied by the following:—

I have a flint, in the centre of which is found, by a happy fracture, a jaw on which are made visible, by the fracture, at least sixteen teeth (see figure). They are very minute; the largest not exceeding in size the point of a pin the eighth of an inch in length. On examining this very interesting specimen under the microscope, it is found that, though the jaw itself is perfect and not silicified†, the pulp cavity of each tooth in which the fracture has exposed it is entirely *filled* with solid flint. How did that flint get there? Geological phænomena will indeed be easily explainable if we are to believe that any sponge gemmule ever found its way into these minute and perfectly inaccessible spaces, and still more that, having found its way there, it was able to exist and grow there without any possible access to the very essential means of its existence, the sea-water. That the silex when in solution possessed however the power, and that it was one of its qualities, to penetrate some of the most intimate tissues of organic bodies within its range, will appear from the next class of facts cited; in reference to which, as well as to the last-named facts, it may be stated that soft animal matter appears to have had a greater



Jaw of *Enchodus* (?) in flint.

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* Odontography, p. 261. and pl. 72; and a section is figured in the London Geological Journal for Sept. 1846.

† See this phænomenon alluded to in a very interesting paper by Dr. Mantell in the 'Annals and Magazine of Natural History,' vol. xvi. p. 80. The whole article is well-worthy of attentive perusal. The same principle will consistently explain why, as often seen, though a Ventriculite was enveloped in flint, the shells on it were left bare, not having so much affinity for the siliceous fluid as the soft animal substance of the Ventriculite. On the sponge theory this frequent fact would be utterly inexplicable.

affinity for the silex than bone or shell; through which latter the silex would frequently pass without lodging there in order to reach those parts for which its affinity was greater*.

We now come to the innumerable cases in which organic remains belonging to the many varieties commonly, though very loosely, grouped as Ventriculites, whether cyathiform, flexuous or quadrangular, and Choanites and the like are found imbedded in flints; and many of the most delicate of which we now find imbedded in the solid flint with yet a light floating elegance of form as if still enjoying life in their native liquid element; and which facts assure us that they were thus suddenly and instantaneously fixed in a moment of the highest vitality.

What are the conclusions to which a careful and extensive examination of these objects necessarily leads?

First, that the animals were enveloped *while living*†, *and not after death,* as the sponge theory assumes as a necessary postulate. The specimens are found both in flint and chalk in precisely the same conditions. They are found in all states, from the fully expanded to the closely contracted. They are found—not, as is the case with the sponge tissues, rarely or never, but—very generally in a perfect state of preservation, the whole tissues and structure being preserved to us in a condition fully as clear as if the living being were before us. Now it is very easy to conceive of the living animal being suddenly enveloped in a mass of soft mud or other liquid which hardened more or less rapidly, but we cannot conceive of its allowing the growth over it, while living, of a sponge which enveloped without killing it or altering its appearance; still less of that sponge afterwards decaying and leaving the inclosed body undecayed.

Second. It is beyond a question that the cause which gave rise to these flints was not a foreign body *enveloping the exterior* of the Ventriculites and other bodies, but a substance which possessed the quality of penetrating their most intimate structure. It is only necessary to examine polished specimens of either Choanites or the cyathiform, quadrangular or flexuous Ventriculites, or the specimens of either cracked open, when, as is often the case, they admit of this, to be fully satisfied on this point. The structure of these bodies themselves remains perfect and most beautiful, while no enveloping structure or tissue can be made out, except in the occasional fragments already noticed. Again, it is not unfrequently found that the flint itself has *only* penetrated the internal structure, none being found encasing either the external or internal surface*. This would clearly be inexplicable on the sponge theory, but presents itself as a frequent phænomenon in several of the classes of objects above enumerated, and is consistent with the observed fact, already noticed, of the affinity of the silex for soft animal matter. When, moreover, flint is found enveloping the outside of a Ventriculite, it is almost invariably found enveloping it, to exactly the same extent and no more, on the inside, and this without the edges of the Ventriculite being at any point overlapped. It is obvious that no sponge can have possibly grown in this manner. Again, the delicate roots of these bodies are very frequently indeed, *more frequently than any other part,* found preserved in flint, a fact in most diametrical opposition to the very fundamental and necessary postulates of the sponge theory, as the sponge would clearly lie upon, not penetrate into, the mud. Yet these ventriculite roots are thus commonly found, while it is admitted that no place of root is to be found belonging to the so-called sponge masses themselves. I have a specimen in which, in one block of chalk, there are *five* Ventriculites. The stem and root of *each one,* and no other part, is enveloped in flint.

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* The same point is well illustrated by cases of fish and crustacea, of both of which I have specimens of which the integument remains unsilicified, the interior is solid flint.

† This might easily be shown to have been the case with other classes of objects, but space forbids extending the illustrations. All these soft parts are preserved, and yet the enveloping sponge has decayed!

It may here be noticed as an interesting and important fact, that it will be found, on careful examination, that not only is the internal structure of the Ventriculite preserved, but, in fine specimens opened with care, the integument of the animal, which presents quite a different structure, is also preserved, covering over the internal structure; and thus at the same time disproving the alleged sponge character of the Ventriculites†, and showing that, at the time when the silex aggregated round and penetrated the Ventriculite, this integument was unbroken. So in Choanites, which have quite a different exterior, that exterior will be frequently seen perfect, clear and distinct, while exterior to it is a layer of flint.

It will be obvious, that if the sponge theory is to be taken as any explanation at all, it must assume, in addition to the inadmissible postulates already noticed, the inconceivable phænomenon that every one of these living bodies was penetrated by the sponge, since the very object of the theory is to show that all the spaces now occupied by flint were once sponge, that is, that silex has precisely replaced sponge and *nothing else.* If it does not go this length it is of no value whatever as a theory, since there is no doubt, as already seen, of sponges being, like other remains, sometimes inclosed in flint: the point is to explain upon a general and *universally applicable* principle, the presence, in all its forms, of flint; and this has been seen by the extract already given to be the express intention of Mr. Bowerbank. If therefore it appears that it is impossible, in any one particular instance, to explain the presence of flint on this theory, the theory itself falls wholly to the ground as an explanation of the phænomena. Its insufficiency is not any more demonstrated, though it is further illustrated, by multiplying such instances or sets of instances. Such illustration is afforded by the variety of facts which have been already cited, and the citation of which, did space permit, might be extended almost without limitation. It is conceived however that it has been sufficiently shown, *first,* from the condition of the assumed sponge remains themselves, *second,* from the nature and condition of the inclosed organic bodies, that the sponge theory is in nowise tenable as a comprehensive theory to explain the origin of the forms and modes in which flint is now found in the chalk.

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* It is worthy of particular remark, too, that all these bodies are very frequently found only partially enveloped in flint, the remainder of the body being found in the adjoining chalk. This affords clear demonstration against the sponge theory, but space will not allow of my entering on this very interesting class of facts.

† See Prof. Ansted's paper above cited.

We will now take another ground apart from any internal examination, to which we have hitherto confined ourselves, of the substance of the flint. And we shall find, on taking a careful review of some facts of the *external* forms and modes in which the flints are found, that the sponge theory is not only wholly unsatisfactory, but absolutely impossible; while we may gain some hints as to the true origin of these masses. To avoid extending these remarks to too great a length I shall confine myself to a few cases, which however are conclusive, and all of which, with many others confirming them, have been collected by myself upon the spot; and I purposely select such as have been collected at spots many miles apart.

The reader is requested to examine carefully the specimen represented in fig. 1 of Plate I., which is, with the others there figured, of the natural size. It will be seen that the external surface of this specimen is entirely covered with strong liny ridges. These ridges overlap one another precisely like pieces of card-board cut to a clear square edge, or, to take another and I believe an exact analogy, as flakes of ice just on the point of congelation would do if violently agitated*. They rise one above another about, on an average, the twentieth of an inch, and, as it will be seen that they run in various directions, they thus give to the surface a very peculiar form, the anticlinal axis *b c* crossing it, and there being at *a*, where the ridges run concentrically, a deep and very accurately rounded cup, of which a small space at the bottom is flat and all around the ridges rise regularly and concentrically; giving necessarily the idea of some liquid substance, of which the surface was very rapidly solidifying, and disposed altogether to very rapid solidification, and which at the exact moment of such condition was affected by some small rapidly revolving eddy, while the other parts of the solidifying substance were at the same moment affected by small but rapid currents running in opposite directions. This very motion probably caused the solidification of the mass, for, curiously enough, there are fewer marks of organic remains in this and another similar flint than in any others I possess*. The edges of all the ridges are sharp, although in this instance, contrary to what we shall presently find in another, the whole surface, under the angles of the ridges as well as above them, is covered with the mealy coating. The under surface of this flint is covered with ridges precisely in the same manner as the surface figured, and in a manner which renders the specimen still more striking as illustrating the action of the supposed currents. Traces of the flakes which have thus been forcibly slipped over each other are also observable in the substance of the flint, at a cross fracture, precisely as might be the case where flakes of ice, slipped over one another, varied slightly in colour, from some contained substance, while separately exposed and before being piled over by a mass of other liquid, on whose thus forcibly and rapidly upraised surface the broken edges of other flakes would of course be seen; and the whole of which mass, so piled up and very rapidly solidified by the very agitation thus given, affords the most exact idea of the present specimen. I have another specimen presenting exactly the same general characters of liny ridges with sharp overlapping edges, though the form assumed is different. I have also a most interesting section of flint, presenting, under the microscope, evidence of a precisely analogous train of facts and consequences. This specimen, which is here figured, speaks for

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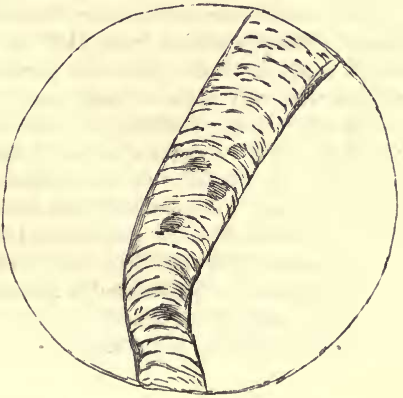
* In an experiment with Glauber salts, made since the above was written, I found, on an accidental motion given, exactly the same character of ridges assumed.



Traces of revolving particles in flint magnified about four linear.

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* "The slightest disturbing causes, as *agitation*, change of temperature *or* the affinity, though slight, of some other body for the solvent, would put an end to the solution."—*Turner's Lecture*, as before cited, p. 26.

itself. The lines on it present no trace of organic structure, but are obviously caused by the rapid revolution of the particles, to which motion the flint, almost free from trace of organism, probably owed its solidification.

These facts are obviously totally incompatible with the sponge theory. Not only is it clear that no sponge can ever have taken such forms of growth as these, which are obviously the result of mechanical causes acting, not on an animal substance but on an inorganic one; but, while the sponge theory, necessarily assuming as it does the decomposition of the saturated sponge, requires as a necessary postulate a very slowly solidifying body, as the only means of accounting for that decomposition, the facts above named obviously point to a very rapidly solidifying substance, a condition, too, best agreeing with known phænomena. Nor do the facts admit of that substance being of the nature suggested by Dr. Buckland. They seem, on the contrary, necessarily to indicate a substance which, while of a considerable specific gravity (flint is 2·59), was in an extreme state of liquidity* for the time, but liable to extremely rapid solidification; in fact such a substance as Dr. Turner indicates. The sharp edges, as well as the character of the ridges, preclude the supposition of a viscid fluid. Phænomena analogous to those suggested as explanatory of the condition of fig. 1 are often seen in cold weather in a vessel of water in which fine flakes of ice only are seen while it is at rest, but the moment it is agitated the whole becomes a solid mass. Similar phænomena may be produced experimentally in many ways.

The rare occurrence in the flint of instances like those just stated probably arises from that very rapidity of solidification which explains those particular instances; a solidification which contact with any organic body would equally induce, and which would of course, therefore, prevent frequent liability while yet liquid to such casualties as there happened. That rapidity of solidification,—a fact, as has been seen, inadmissible on the sponge theory,—is demonstrated not only by the facts last stated but by those to be next named, and which, with the last, render it indisputable that the flint was perfectly solid and brittle (and not in anywise in the state of a glutinous or viscous mass) before the chalk itself became solid or was in otherwise than the state of a soft mud, but yet while it was in a state of mud and not merely of muddy water *. This is a very important point, as it enables us to explain very many of the most striking phænomena connected with the deposit and contents and aspect of flints.

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* Dr. Turner speaks, p. 27, of the silica being in a gelatinous form and hardening slowly. Why this should ever have been assumed does not appear, and both parts of the assumption are obviously directly inconsistent with all the general principles enunciated in that lecture and with observed facts, as above shown. There is probably some incompleteness in this part of the report of his lecture.

Pl. I. fig. 2 represents a specimen extracted with elaborate care from the rock with my own hand, and in which therefore no mistake exists as to facts or appearances. It is a mass of chalk in which is a flint in part enveloping a Ventriculite, though a part of it (on each side of *d*) is an instance of a frequent case above named, where the flint is found *not* enveloping but only penetrating the substance of the animal; and the specimen affords the yet further illustration of a Ventriculite preserved partly in flint, partly in chalk. Now this flint formerly enveloped with an extremely thin coating (not the eighth of an inch thick) the lower part of the Ventriculite, together with the stem and roots. *After* it was thus encased however, and while the chalk was yet a soft mud, the flint, then necessarily solidified and brittle, was by some accident broken. The force which broke it nearly expended itself in so doing, and was not sufficient to overcome entirely the adhesive properties of the mud and so to remove the separated parts to a great distance. In this instance there were in the chalk, when I found it *in situ,* four pieces of flint *a, b, e,* and one at *c,* the impression of which last remains, but the piece itself was broken out before I discovered the nature of the specimen: *a, b* and *e* remain exactly as they were. Neither of them has ever been disturbed, and each is perfectly fixed and firm in the chalk. The chalk still remains, filling up the spaces between them and between *c* and *a,* except where I have, to a slight depth, cleared it away between *a* and *b* in order to ascertain the interesting fact to be next stated, namely, that the substance of the structure of the Ventriculite is perfectly preserved and displayed in each piece *a* and *b,* and *the edge* a *is the exact counterpart of the edge* b, *and would fit to it if they were brought in contact.* A more interesting fact it has seldom been my lot to develope in the course of my geological researches. This case alone is sufficient to establish beyond possibility of dispute the proposition to illustrate which it has been cited. Neither the mass of chalk in which it now exists, nor that from which it was extracted, does or did exhibit, on or near the place of this fracture, the slightest trace of any movement or displacement. Being, as above said, obviously in a state of soft mud when the accident happened, it was sufficiently adhesive to retain the pieces in these positions and to prevent injury to the exposed parts of the Ventriculite, but filled up the space made by the fracture, and afterwards gradually hardened, bearing itself no trace of the event.

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* This condition, while thus demonstrated, explains with a beautiful consistency the very frequent fact of Ventriculites, &c. preserved half in flint and half in chalk. I regret that space will not allow me to enter on that point, which an extensive reference to facts only may render obvious to those unfamiliar with the phænomena.

I have also another and larger specimen in which precisely the same state of facts is found, and which therefore, though an important illustration of the results learnt from the last specimen, it is unnecessary to describe in detail.

Fig. 3 of the Plate exhibits phænomena of the same character, but carried further. A mere glance at the figure will show that the mass of flint is covered by sharp angular pieces of broken flint adhering to its surface. These do not, as in the case of fig. 1, rise as mere ridges. They are clearly broken fragments of a flint already solidified, which have here lodged on this mass while in the act of solidifying, and have become a part of it. The places of union show no mark of junction or fracture: they are completely united. The edges are sharp enough to cut the finger, and the mass *c*, the largest, and which is itself, as will be seen, cross-cracked into four pieces which are now at slightly different elevations, rises at least the sixth of an inch above the mass onto which it is fixed. The edges of these pieces are *not* covered with the mealy coating*. It should be remarked that the minute angular fragments scattered all over the surface of this specimen are much more numerous than could be represented with clearness in the figure, and that the whole mass on which these angular fragments rest (the left-hand portion of the figure) has received a slip over to the right, so as to leave a ridge at *a b* of a quarter of an inch high and having a very sharp edge, and beyond which to the right *not a single angular fragment is found.*

It is clear that in this case a very different state of things took place to what happened in fig. 1, although equally clear, as in that case, that the sponge theory is directly opposed by the facts, as neither could any sponge thus fracture (which, if that theory be true, must be assumed), nor could such broken fragments fall in this manner onto a soft sponge and become thus part of its substance. It is obvious that a mass of flint had become perfectly solidified; that that solid mass became by some accident greatly shattered; and that some of the fragments fell through the soft chalk-mud upon another mass at that moment on the point of solidifying. The movement which caused this fracture and impelled the pieces onto the yet fluid mass was probably the same which caused the whole surface on which the fractured pieces alighted to slip forwards, and which surface and the mass beneath it, probably by the very agitation thus caused, instantly solidified, leaving the ridge *a b*, and fixing the fractured pieces firm.

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* Thus disproving a view which has been advanced, that the mealy coating is the mere effect of exposure to carbonate of lime and the deprivation of some of the water of the flint thereby. These parts have been in every respect equally exposed with all other parts, but were not so until after they had become solid. The mealy coating does not seem to present any difficulty, but space will not allow me now to enter into it further than to add that I have found it covering a large part of the surface of flints inclosed in Echinites.

This case illustrates and demonstrates all the conditions already noticed; extreme liquidity and rapid solidification of the flint, together with the soft state of the surrounding chalk.

It is unnecessary to enter now into further details of facts, or to dwell long on the conclusions to which the numerous facts above cited lead us. Some of these conclusions have been suggested in the course of these observations, others will naturally suggest themselves *. Numberless facts unite to show that the sponge theory is wholly untenable, while they point to a state of facts easily conceivable and analogous to what is found in other formations; which is in accordance with known laws and experimental phænomena; and with which all the observed phænomena of flint will be found consistent. For different mineral concretions to form themselves round organic centres, by the combined effect of some affinity thereto and of their own molecular attraction, is a phænomenon of frequent occurrence. In the Septaria of the London clay, each having its organic centre, numerous organic remains are found interspersed, and frequently half imbedded and half projecting on the surface, precisely as is the case with the flints. The siliceous solution, which was probably supplied periodically only, being supplied but to a limited extent and during a certain part of the chalk formation only, differing, as is known, in different localities, could only form itself into masses which occupy a certain space. Whether it took the form of nodular or tabular masses was owing to local modifying causes. Where organic remains of any considerable size, or grouped in particular masses, happened to be abundant and lie near one another, they acted as separate centres, while the solution was attracted to them in a mass. Hence the fantastic and varied forms in which we find the nodules, together with the projecting bodies, the supply of material being often too limited to encase the whole of all the organic bodies by which it was attracted. Where those bodies were less abundant or less individualized,—as where there was a layer of minute organic bodies,—there would be fewer centres of attraction, and tabular flints would be formed. Wherever the concretions happened to form themselves they would be attracted by and envelope, more or less, every organic body with which they came in contact, living or dead. That the siliceous liquid possessed the power of penetrating,—that is, had some peculiar affinity for,—the substance of the organic bodies, vegetable or animal, which it enveloped, and even sometimes without being in sufficient quantity to envelope them at all, is a fact which we see before us. This may be a fact difficult to explain, but it is one which is exemplified in innumerable other instances, and which is not therefore peculiar to flint *.

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* I conceive it unnecessary to enter here at length on the question of the arrangement of the flints in layers. Such a phænomenon presents no greater difficulty than any fossiliferous layers proving several distinct and successive bottoms of the sea; such, for example, as form so marked a feature of the Dudley limestone. There would thus always be some bottom sufficiently hard to resist the pressure caused by the specific gravity of the siliceous fluid and flints, the greater part of which would thus fall onto it through the softer mud above. Scattered small flints are found between the layers, which a variety of causes may have arrested in their descent. The distances between the layers are by no means equal; the modifying conditions which caused at intervals, in this formation like others, a relatively hard bottom to form, varying at different times. That such "modifying conditions" must be taken into consideration will be evident from the important facts that in the Lower Chalk no flints at all occur in the south-east of England, while in the Middle Chalk they do occur scattered indiscriminately through the mass; but it is only in the Upper Chalk that they assume the peculiar stratified arrangement which marks it. To explain these facts on the sponge theory would indeed be impossible, but they are perfectly consistent with the conclusions above drawn, and those conclusions and these facts mutually illustrate each other.

Such are the conclusions to which a very careful and extensive observation of the phænomena of flints has led me. Accepting the views of Dr. Turner, combined with the well-known facts of the products of thermal springs, as affording a satisfactory and the most probable explanation of the *origin* of the siliceous liquid, I offer these conclusions as explanatory of the modes and forms in which we actually find the flints themselves: and I offer them without being wedded to any preconceived notion or theory, but simply as the results of extensive observation, and as alone appearing consistent with all the varied class of facts which that observation has brought under my notice.

Highgate, Nov. 1846.

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* The extreme liquidity of the siliceous fluid, combined with its specific gravity and the superincumbent pressure, help to explain these facts. The questions of that specific gravity of the siliceous fluid, and of its not undergoing any material contraction when solidified, cannot be here discussed, though illustrating and supporting the conclusions to which other facts have above led.